IMPERIAL BUREAU OF PASTURES AND FORAGE CROPS

ABERYSTWYTH, GREAT BRITAIN

Mimeographed Translation No. 1/1938

Fourth Session of the
Academy of Agricultural Science, Moscow, U.S.S.R.,
December, 1936

[Extension of colloquium read by P.C. Koller at the Institute of Animal Genetics, Edinburgh, June, 1937]

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1. INTRODUCTION.

The fourth session of the Academy of Agricultural Science, held in Moscow from December 19 to 27, was devoted to plant and animal breeding and those questions of general genetics which since the report delivered by T. D. Lysenko on June 25, 1935, were the centre of vivid, and occasionally passionate, discussions through scientific journals in the Union. The session was opened in the afternoon of December 19, the opening address was given by A. I. MURALOV, the President of the Academy, who stressed that "the session held must bring clarity to a number of disputable questions and give the required and correct trend to further research on breeding and genetics" and that "the questions under discussion should be considered not abstractly or scholastically, but concretely and with reference and with a view to that comprehensive practical research which is being carried out" in the Union. Muralov points out that the discussion held is of particular significance, as in genetics "stagnation and anachronism" hampering breeding work, had frequently occurred. "If it will be necessary", Muralov continues, "as a result of our discussion, to revise the outlook and old methods and replace them by the new, our scientific workers will not hesitate to do so", bearing in mind that "the science is called science because it is not afraid of any fetish, or of lifting a hand against the obsolete and old ideas and attentively listens to the voice of experiment and practice".

After the opening address the session commenced its work in which over 700 persons, and, in some instances, as on December 22 and 23, over 3,000 persons, were present. The following programme of work was adopted.

(1) Reports of experimental research on building up new varieties of cultivated plants and new breeds of domestic animals. Each reporter was given 15 mins. Of 147 reports received, only 50 were chosen for reading, but eventually 72 were read. These reports were read during the remaining part of the evening meeting on December 19, during the morning and evening meetings on December 20 and 21, and during a morning meeting on December 22. An agricultural show comprising agricultural exhibits, schemes, diagrams, maps, etc. arranged in the buildings of the Academy, materially demonstrated and illustrated the reports delivered.

(2) The main reports on the theoretical questions of genetics and breeding. The hour allowed for each of these reports was later prolonged to $2^{1}/2$ hours. The reports were delivered during evening meetings on December 22, by N. I. VAVILOV, Vice-President of the Academy and Director of U S S R. Institute of Plant Industry, Leningrad, and on the following day by T. D. LYSENKO, Member of the Academy and Scientific Advisor to the Institute of Plant Breeding and Genetics, Odessa; A. S. SEREBROVSKII, Member of the Academy and Scientific Advisor to the U.S S R. Institute of Animal Breeding, Moscow; and by Professor H. J. MULLER of Texas University, and Corresponding Member of Academy of Science in U.S.S.R.

(3) Discussions on the reports which were begun on December 25 and concluded on December 27. Over 100 persons reported for discussion, but only 42 took part. No meeting was held on the afternoon of December 23 and the whole of December 24.

After the discussions time was granted to Muller, Serebrovskiĭ, Lysenko and Vavilov for answering their opponents; and, finally, G. K. MEISTER, Member of the Academy and Scientific Advisor and Director of the Saratov Experiment Station, read a report in which the results of the discussion held on theoretical questions of genetics and breeding were drawn up. The session closed with an address delivered by Muralov.

Omitting all the plant breeding reports which are of less general interest to give some extra space for Lysenko's report, below is given a brief account on the main reports and on some most interesting parts of the discussions thereof, as could be drawn from those scanty and incomplete materials which have appeared by now in Bull. Vsesojuz. Akad. Seljskohoz. Nauk, Nos.1 and 2, 1937, Selekcija i Semenovodstvo, Nos.2 and 3, 1937, Problemy Životnovodstva, No.2, 1937, Jarovizacija, No.1(10), 1937, and Social. Rekonstr. Selj. Hoz., No.2. 1937

2. THE MAIN REPORTS ON GENETICS.

The report read by N. I. VAVILOV.

Plant breeding in the Soviet Union as distinct from U.S.A. and Canada, at an early stage was based on the use of indigenous varieties of cultivated plants. The introduction and research with foreign varieties was begun mainly after the disastrous year, 1922, since when all the best of "the world collection was, as it were, subjected to the Soviet test". As a result, about 15 per cent of the present total sown area are under the best foreign varieties. In the following years, intervarietal hybridization was carried out on an ever increasing scale. The most conspicuous success in the majority of cases is connected with the hybridization of contrasting, most widely differing forms.

The comprehensive studies of gene representation of cultivated forms enabled investigators, on the one hand, to compile a preliminary theory of initial material, and, on the other hand, to disclose the fact that the material at the disposal of the European, Soviet and American plant breeders is incomplete and mostly occasional. This gave the impulse for the search for new progenitors and forms of cultivated plants, both within and outside Soviet territory. About 60 countries had been visited by Soviet expeditions. As a result of this comprehensive agro-botanical survey, extensive material had been collected, the

greater part of which was discovered for the first time.

Subsequent years had been devoted to a complex study, first of the systematics of that collection, which as such has hardly any precedent in the history of science. A serial publication which is being issued under the general title of "Cultivated flora of the U.S.S.R.", summarizes this research as far as the interests of Soviet breeding are concerned. The species of cultivated plants, contrary to the previous conception based upon incomplete material, proved to be complex systems differentiated in their evolution into a number of eco-geographic groups. The differences mainly concerned such agricultural characters as vegetative period, developmental stages, immunity, resistance to drought and frost, etc., in the expression of which an eco-geographic regularity common to different plants, could be detected. This enables Soviet workers to complete and revise the eco-geographic characteristics of the agricultural regions, from which the initial material was obtained.

In order to give a sound basis for the conscious choice of parental forms, as a result of thorough studies, special tables were compiled giving all the plant breeding potentialities. At the present time these tables have been completed for eleven of the most important plants.

The comprehensive materials collected open a new field for intervarietal hybridization, over 160 breeding centres dealing with the collection of initial forms. Apart from other methods,

inbreeding is being widely used, both as a method of obtaining new and better balanced forms and for close-related reproduction. Although there have been cases of successful application of inbreeding to obligatory cross-pollinating plants, yet this method cannot at present be recommended in work with these plants, as the low seed yield and depression of vigour in the hybrids hamper the progress of building new strains. The efficacy of inbreeding increases, however, with the faculty to self-pollination.

One of the divergencies with the Odessa workers consists in the evaluation of inbreeding. The opinion as to the harmful effect of inbreeding on self-pollinated plants has not been substantiated, neither is there any support for the genetical degeneration of pure lines. For example, there is in U.S.A. a variety of "Mediterranean" wheat introduced in 1819, or Turkey, a Crimean wheat, introduced in 1873, or, in England, Chevalier barley bred from a few seeds in 1823, which show that there are many pure lines which do not degenerate or deteriorate in quality, in spite of continuous cultivation for some 50 to 100 years.

The conception of pure lines is relative; the lines were built up in respect of certain characters, in other characters they might be heterozygous. In such a case the effect of intra-varietal crossing suggested by Lysenko is quite possible. Heterosis may also account for Lysenko's results. Nevertheless,

the application of this method cannot be recommended, as it is not yet known how other pure lines might respond. A correct seed production and the agronomic methods of maintaining the purity of a line are by all accounts more effective and certainly less risky than the method of intra-varietal crossing.

Distant hybridization has also been repeatedly tried in U.S.S.R., since the successful attempt made by Mičurin. Despite the advances made in this field, for example, determination of a homology of chromosomes in different species (Meister, Veruškin, Cicin and others), or the possibility of overcoming hybrid sterility by doubling the chromosome number (Karpečenko), distant hybridization is still confronted with serious difficulties; in the meantime not by any means all the possibilities of intra-specific hybridization within the world collection of forms have been exhausted; for this reason, but without any desire to undervalue inter-specific and inter-generic hybridization, greater attention should be devoted to intra-specific and close-related hybridization.

Methods of inducing mutations have also been studied, but so far the results have not been up to expectations; there is no reason to recommend these methods which are still in the experimental stage, when even intra-specific hybridization has not been fully employed.

* * * *

In his concluding report, Vavilov dealt with various questions raised during the discussion. He emphasizes that the laws of homologous series are free from any concepts of vitalism whatever, as the laws are "a formula of accurate facts based in all cases upon evolutionary doctrine". At the same time, being a product of the contemporary state of genetics, they are not perfect. In the first place, the laws do not account for possible gene mutation, as at that time the gene was thought to be more stable than it now is after Muller's research. It was also believed then that many genes must be identical in closely related species, while it is now known that these species, in spite of a number of common characters, might have different gene representation. The laws must therefore be revised and brought up to date; but, incomplete as they were, they were of great service in the past. At the present time there is no doubt about gene mutations, but on the other hand, Lysenko's claims that the gene can be changed in a desired direction by "training the plants" require more accurate experimental tests.

The theory of the centres of origin of cultivated plants is similarly not without its own defects. Here again, the obsolete ideas of gene stability have left a deep impression. It was then thought that the genes differentiating modern plants are localized in the centres. As a matter of

course, a great many genes had been found in the centres, and yet many new genes, some very valuable, were found elsewhere outside the primary centre.

In the theory of parental pairs, Lysenko's theory of plant development received due consideration. Nevertheless, his conception as to the F_1 generation being a criterion for subsequent generations in breeding for earliness was not confirmed in many comprehensive experiments, as in many cases some of the segregates in the F_2 and F_3 generations proved to be earlier than the F_1.

Vavilov points out that the alterations of gene representation by "training" must be further tested, while the conception of adequate changes contradicts the entire experience of modern genetics.

In conclusion, Vavilov stressed that "We have not convinced one another, but the differences have now taken a clearer shape and our outlook is mutually and adequately understood", but he regrets the lack of understanding and respect towards a fellow scientist.

The report read by T. D. LYSENKO.

It is emphasized that the main causal difference between orthodox genetics and himself consists in the discrepancy in the outlook on the fundamental principles of

evolution established by Darwin. Lysenko claims not only correct understanding, but also a further development of the fundamental conceptions of Darwinism and accuses his opponents of misunderstanding and perversion of the firm foundations of Darwinism. Summing up a secular experience and knowledge of mankind, Darwin, argues Lysenko, established a creative rôle both in natural and artificial selection. Natural selection operates on those characters and properties which are useful to the plant in the sense of its greater adaptability to the environment, whereas artificial selection is made by man, who selects only those individuals which best meet his requirements. Some of the changes induced by man might cause death of the plant, that is, natural selection might operate side by side with artificial selection. However, while changing the nature of a plant, man by appropriate agronomical means also changes the environment. Thus, the higher and more intensive is the cultivation of the plant, the greater is the rôle of artificial selection and accordingly the lesser is the rôle of natural selection in building up new forms. Darwin had thus given not only a sound interpretation of the apparent fitness of plants and animals to the environment and the requirements of man, but also had given the man a scientifically sound basis for building up new forms. Darwin's doctrine was severely condemned and criticized by

geneticists who have severally challenged the reliability of Darwin's approach and contemporary scientific data. De-Vries and his supporters, for instance, opposed Darwinism with the mutation theory, as if Darwin was not aware of fluctuating changes; Johannsen, advancing the theory of pure lines, defied, or at least, restricted one of the fundamental concepts of Darwinism, namely, the creative rôle of selection.

Lysenko claims that the conclusions formed by Johannsen are not well founded. Firstly, he used an inadequate number of plants; in his six-year research with beans, he obtained 1525 grains or about 800 grains per variant, selecting yearly 2-5 grains for each class from about 150 grains. Secondly, this selection was made from the yield of all variants lumped together, and not from seeds obtained from individual plants; it is known that the size of seed varies not only between plants, but also even within the same pod, and cannot always be used a direct measure of the nature of the plant, as such. Had he used a larger number of plants and applied individual selection from the extreme variants, Johannsen could have arrived at conclusions differing from those which confirm the inefficiency of selection among self-pollinating plants originating from the same seed. It is true, argues Lysenko, that Johannsen's principles are rarely, if ever, used in every-day practice even by his own followers, and yet, referring to some quotations from Morgan's latest book,

Lysenko alleges that modern genetics has not yet repudiated the conceptions formed by Johannsen.

Two reasons compelled Lysenko to raise the question of the revision of certain principles of genetics, namely, (1) improving the quality of cultivated self-pollinating plants by means of a systematic intra-varietal crossing; and (2) alteration of the nature of plants by growing them in an appropriate environment.

Lysenko referring to Darwin, maintains that, contrary to the opinion of orthodox geneticists, pure lines when cultivated for a long time degenerate, i.e. deteriorate in quality. In some plants, for instance cereals, this degeneration goes on slowly, while other plants, for instance tomatoes, degenerate in 3 to 5 years. As to the cause of this deterioration of self-pollinating plants, Lysenko suggests that with perpetual self-pollination the range of their variability and hence of their adaptability to the environment becomes narrowed, as compared with plants derived from intra-varietal crosses. The germ cells, argues Lysenko, accumulate and retain all the abilities gained by the preceding generations, and particularly by their nearest ancestors. In self-pollination the participating male and female gametes, while developing on the same plant, undergo identical modifications; for this reason they contribute the same abilities to the zygote. In intra-varietal crossing the male and female

cells develop on the plants grown under somewhat diffe conditions and hence are modified more or less differently for this reason, in fertilization they enrich the zygote, enlarging thus the adaptability of their offspring to the environment. In subsequent self-pollination of these plants, the differences previously inherited from the male and female plants gradually vanish with generations and, consequently, the range of their adaptability to the ever-changing environment becomes accordingly narrowed; hence these rigid plants do not endure a long cultivation; they degenerate, deteriorating in their agricultural value. In nature, as Darwin had showed, self-pollinating plants retain their stability through cross-pollination; in nature not a single plant is known to be deprived entirely of the ability to inter-cross. In nature, however, all the seeds are intended for reproduction, but only a small portion of them survives and there is no ground to deny that a considerable part of the surviving individuals arise from cross-pollination. It is different with cultivated self-pollinating plants; only a small percentage of seed yield is used in sowing, thus decreasing the chance of a natural rejuvenation of the variety. Following upon these premises, Lysenko suggested in 1935 the introduction into practice of systematic intra-varietal crossings, eliminating of course any possible contamination. Numerous experiments carried out in various parts of the Union in which both research

workers and farmers participated, have fully corroborated the expedience of such rejuvenation, for in the first three generations these plants showed an appreciably improved yield and winterhardiness. Geneticists, who in 1935 denied any possibility of rejuvenation of a pure line, when aware of the results obtained by intra-varietal crossing in practice, now interpret the increased growth vigour in terms of heterosis, i.e. by a growth vigour. They fail to give a real interpretation, they speak in circulus vitiosus, as with mutation.

Concerning the second problem, i.e. altering the nature of a plant in subsequent generations by growing it in an appropriate environment, which is referred to as 'training', Lysenko first denies the accusation of Lamarckism, as in his experiments he has not been proceeding from the concepts of Lamarck. It is universally recognized that environmental factors play a profound rôle in evolution; although geneticists argue against the possibility of directing hereditary changes by the environment, yet it is hardly possible to deny the profound effect of environment on morphogenesis. Nevertheless, there has hitherto been no experimental data to suggest what these environmental factors are, when and at what developmental stages of the plant they must operate in order to change the hereditary nature of the plants in a desired direction. Study of the biology of plant development in earlier years enabled Lysenko to establish "bio-stages" at

which the plants exhibit definite properties, definite demands and definite responses to the environmental factors. As the first of those stages (the stage of vernalization) is best known, Lysenko began his experiments on altering the nature of plants, on the basis of the response of plants to the environment at this stage.

It is known that at that developmental stage winter and spring wheat forms differ in their requirement to temperature; it is also known that the most suitable temperature for the winter forms to complete their first stage is 0 to 2°C., but they are able, although at a much slower rate, to complete this stage at temperatures of about 15 to 20°C.

Vernalizing processes, i.e. some internal readjustments, presumably in the protoplasm, during the first developmental stage, occur in meristematic cells, from which later the tissues of stem and leaf as well as of the floral organs with their germ cells arise and become differentiated. Therefore, these cells are, as it were, the basis for the whole subsequent morphogenesis terminating with gametogenesis. Lysenko assumes that the meristematic cells of two plants of the same variety will be different, if these plants are forced to complete their first stage at different temperatures. Furthermore, he postulates that this qualitative difference between the meristematic tissues is retained and transferred to all generations of cells developing

from them, including the germ cells, i.e. this difference is retained throughout morphogenesis; for this reason, the seed produced by a winter plant grown at high temperature will complete the first stage easier and more rapidly at high temperatures, and the subsequent growing of further generations will more and more alter the winter plant into a spring form.

Proceeding from these theoretical premises, experiments have been in progress since 1935 on altering winter into spring plants and, vice versa, growing winter plants and their offspring at temperatures higher, and those of the spring forms at temperatures lower than the optimum. From his brief account on these experiments with winter cereals, it may be gathered that even the third generations of plants thus "trained" behave as spring forms, earing 30 to 50 days earlier than the preceding generation at temperatures never falling below 15°C. In the second and particularly in the third generation, the change in the first stage towards the spring type was associated with changes in morphological characters, such as ear, glume, length of awn, etc.; in the third generation some narrow-leaved forms appeared.

The results obtained in training winter forms gave grounds for attempting to convert spring forms into winter by growing them during the first developmental stage at temperatures lower than the optimum. Such a training is of greater practical

value, as with prolongation of the first stage winterhardiness and the ability to "hardening" are increased. The results obtained corroborated fully the possibility of such a training of spring forms.

Using an appropriate method of training some attempts are being made to train thermophyllous cotton into "cold-loving" types. It is claimed that appropriate training will enable breeders to change the plants requiring long or short day at one of the developmental stages. It is pointed out that training of plants was particularly easy with heterozygous forms, such as rye.

Lysenko is fully aware that the data obtained in research on training plants are incompatible with the concepts of modern genetics; he thinks, however, that neither his interpretation, nor the data reported are in disagreement with some facts reported by geneticists; on the contrary, he claims, they explain them. Modern genetics, continues Lysenko, holds that some self-reproductory "corpuscles" localized in chromosomes are alone the physical basis of heredity and are independent of those changes which, otherwise, may involve the whole organism carrying them. Lysenko defies this conception and postulates that the cell, as a whole, is the physical basis of heredity and that every constituent within the cell, including the "corpuscles", is subject to evolutionary changes.

These data, furthermore, contradict the law of homologous series which maintain that the new forms rise not as a result of evolution, but through recombination of previously-existing materials and thus tend to limit the rôle of natural and artificial selection.

Concluding his report Lysenko denies that he is against genetics as such; "genetics, as one of the most important branches of agro-biological science, should be mastered and revised as soon as possible" states Lysenko. Nor does he deny the importance and value of cytology and other branches of science, but doubts "that geneticists together with cytologists can see a gene under the miscroscope".

The report read by A. S. SEPEBROVSKIĬ.

The following conceptions are regarded as conclusively established in genetics and can be taken as principles in practice, namely, phenotypes can be genotypical or phenotypical; organismsproducing similar phenotypes can genotypically be different and vice versa; one or several genes corresponds to each hereditary difference; the gene is a part of the chromosome and transmission of genes is connected with chromosome mechanism; genes can be recessive and hypostatic; heterozygotes produce gametes, which show segregation, following Mendelian ratios; genes

may be combined, and their association may be regulated by crossing; genes are stable biological elements, subject to mutations, after which they regain their stability; frequently mutating genes are very rare; genetics enables investigators to increase frequency of mutation by external factors; induced mutations are frequently different and even mutually opposite; changing gene in a desired direction at the present time is rather speculative; the methods of influencing genes by changes in phenotype are erroneous.

In conclusion he lays stress upon and enumerates contributions of these genetical principles to animal breeding.

The report read by H. J. MULLER.

There is no "formalistic" genetics distinct from orthodox genetics, for genetics today embraces one coherent body of principles on which virtually all geneticists are agreed. The main of those principles is the conception of the physical existence of gene as a body differing from the other constituents of the organism. The gene has been shown to be the biological basis for organic evolution and is a solid material basis for Darwinism that was lacking in the 19th century.

The conception of Lysenko, Present and the others, abolishing the sharp distinction between the gene and the rest of

the organism, between the genotype and phenotype, making their relations mutual and maintaining that genotype bears the impress of developmental manner of phenotype, are irreconcilable with the fundamentals of genetics, as they abolish the most distinctive properties of the gene as such. These distinctive properties are as follows: firstly, the gene reproduces a likeness of itself, that is, the nature of the daughter gene depends entirely upon the mother gene, and not upon the phaene or the surrounding protoplasm; and secondly, although the gene is highly stable, it is liable to mutation, but of such a nature as to leave it still capable of reproducing its own likeness. These mutations are very rare and fortuitous and bear no impress of the changes undergone by the phenotype.

The relative independence of the gene from the remaining constituents of the organism has been ascertained in innumerable studies in which the phenotype has been exchanged or changed by various means. The experiments with pure lines also showed that, starting with a gene complement in the homozygous state, the differences in the mode of development of individuals have no effect on their progeny. The stability of the gene has also been shown to hold in heterozygous individuals. Independently of the phenotype which happened to develop in the heterozygous parents, as affected by the habitat, they give exactly the same segregation

Mathematically it has been proved that the ratio of two classes of progeny, having alternative allelomorphs, is as near to I : I as could be expected on the laws of probability. Moreover, it has been experimentally shown that two out of four gametes produced at the maturation division from each gamete-mother-cell contain one allelomorph, and the other, two, the other, showing thus that this is a result of an inner segregation at the maturation of the material bases received from the parents. All this holds true, irrespective of what phenotypes have been developed in the parents or in their ancestors and of the continuity of their being in that status. Lysenko's opinion then that the cause of segregation lies not in the gene differences received from the parents, but in the habitat in which they developed, is inconsistent with many well-established facts.

Studies of the mathematical relations existing in the heredity of characters showed that the genes for respective characters are grouped and that their linkage in these groups is chain-wise, that is, they form the so-called linear series. It has been concluded also that these series are nothing other than real physical filaments. It has also become known that at the maturation division there occurs at certain points an exchange between these chains. Thus geneticists arrived at the conception as to the structural models of the germinal material and opened up the possibility of forecasting the results of crosses and of

governing this process.

On the other hand, cytological investigations revealed exactly the same picture, namely, filamentous bodies consisting of chains of different parts, each of which reproduces its like. These bodies, i.e. the chromosomes, show exactly the same processes of exchange and segregation as those established for the groups of genes. Moreover, it became possible to identify the individual chromosomes with the individual groups of genes found in experimental studies. This identification has now proceeded to smaller and smaller parts of chromosomes, which has become possible by means of Painter's method of studying chromosomes. The results thus obtained have confirmed the chromosome theory of genes in minute details, and have shown the parts of the chromosomes containing but a few genes.

Further, cyto-genetical studies have shown the generality of the announced principles, that is, the stable Mendelian genes lying in the chromosomes constitute the hereditary basis not only of some special superficial characters, but of all the characters in general, including those differentiating the species. This knowledge has provided grounds for asserting that the gene is a foundation of biological evolution.

In view of the immensely great number of genes in an individual for the success of natural selection, it is indispensable that the gene is very stable, otherwise the species would

disintegrate genetically. Quantitative studies have actually found that a gene mutates rarely, on the average once in many thousand years. Yet, owing to the number of genes, the frequency of mutation is enough for evolution.

Mutations, large or small, occur in many directions in any habitat and it was established that changes in the habitat leave no impress on the direction of mutation, and very rarely has an effect on the frequency of mutation. The evidence shows that the cause of individual mutation is localized to an ultramicroscopic degree, and hence consists of individual molecular changes following the statistical laws of the kinetic theory of molecules. A mutation may occur when a particular particle has chanced to receive energy exceeding a certain critical limit, and the molecular movements are so heterogeneous in any case that whether this occurs is largely matter of chance. The outstanding characteristic of mutations is their sporadic and accidental manner of occurrence. True, many claims have been made of producing directed mutations, but these experiments have not used the exact technique required for critical study of mutation, and none have stood the test of criticism or experimental revision. The latest of these claims made by Lysenko by all accounts is suffering from the same errors as those of the past.

The properties of the gene, its high stability and clean-cut segregation, and the fortuitous nature of its hereditary changes

are the indispensable pre-requisites for a soundly based theory of natural selection; in reality, these work out to give evolution by natural selection of variations that appear to be continuous. But for the efficiency of this process, the sharp discrimination between genotype and phenotype and the relative independence of the former from the latter are the pre-requisites. A clear-cut understanding of this distinction must be borne in mind while attempting to direct evolution to the best advantage.

Study of plant development can be of great value, as it will help us to analyze the characters of the plants and their possible recombinations. These studies, however, concern the phenotype, and the obtaining of new combinations by crossing still depends on the Mendelian principles of the behaviour of the genotype and on their attendant elements of probability which cannot be circumvented.

Confusion between phenotype and genotype may lead to serious mistakes in practical work; for instance, the fruitless attempts to induce mutation in a desired direction or misuse of crossbreeding and breeding, as a result of the erroneous opinion that inbreeding in itself causes deteriorations in the hereditary constituents. Besides, understanding of cyto-genetical principles is an indispensable guide in choice of methods and material, as has been pointed out by those geneticists who are engaged in such work.

There is no question of the abandonment of these principles or of relegating their study to the realm of abstraction, and theoretical work in genetics will remain to provide the basis for

ever more enlightened practice in the future.

3. DISCUSSIONS ON THE MAIN REPORTS.

A number of scientists who took part in the discussion agreed that there is a gap between the theoretical genetics and practice. E. Ja. BORISENKO emphasizes that the question under consideration is not whether genetics is needed or not. Genetics has undoubtedly made rapid advances during recent years, and the chromosome theory can rightly be regarded as an indispensable basis for genetics. Nevertheless, genetics retained some anti-Darwinian, obsolete and incorrect conceptions, which must be immediately discarded.

B. M. ZAVADOVSKIĬ, referring to Muller's report, remarks that Muller seems not to be fully informed as to the aim of the discussion, as the question at issue is not a destruction of entire genetics, but its dissociation from some false conceptions.

A. A. NURINOV thinks that the gap existing between genetics and breeding is due to a persistent refusal of geneticists to consider facts reported in practice.

E. I. SINSKAJA is of the opinion that the cause of this gap is a flaw between genetics and phylogenetics; many questions, including inbreeding, cannot be understood without reference to phylogenetics. The same applies to phasic development. Some instances are now known in which the new forms differ from the initial forms, first of all, in the character of the stages, as has been found, for instance, in the more southerly early forms of lucerne

which originated from the late forms; when grown in short photoperiods, the late lucernes form a compressed rosette, whereas in the early forms the shape of the rosette does not change, showing thus that in phylogenesis a change in the type of stages brought about a change in such a taxonomically important character as the type of rosette.

N. G. ŠLYKOV draws attention to the fact that Lysenko has shown that Mendel's laws do not operate under certain conditions and that the study of such cases might lead to the discovery of some new supplementary laws.

B. M. ZAVADOVSKIĬ considers that attacks against Lysenko are not well founded, as it is not the fault of Lysenko that the facts established by him are irreconcilable with the concepts of modern genetics. The known facts that a gene can be responsible not only for a character, but also for a group of characters, suggest that the conception of the linear arrangement of genes is not consistent and may compel geneticists in the future to seek some new and perhaps more complicated law of heredity. The effect of the environment on gene development must not be underestimated; geneticists, while theoretically accepting the influence of the environment, persistently criticize any attempt to establish the effect of the environment upon the hereditary base.

D. A. KISLOVSKIĬ holds also that an organism must be considered with reference to its definite environment; he considers

that the conceptions and experiments reported by Lysenko open broad possibilities for new research and advances.

M. E. NEVAŠIN reminds the Congress that the aim of the present discussion is to bring genetics nearer to practice; for this certain conceptions must be re-examined, but the revision must be competent and based upon firmly established facts. The existence of the physical bases of heredity, he considers, is proved beyond any doubt. Interference by I. Present and some other incompetent persons can only hamper the progress of this serious undertaking. He would like to hear the call of Lysenko in such a form which would enable him to undertake an experimental study of the problem of 'conversion' of plants. The interpretation, as given at present by Lysenko, leads to Lamarckism; by inducing a change in a character (modification), one cannot hope to fix hereditarily the change obtained. It would be different if an attempt were made to investigate all those phases which a genotype undergoes in development; so far only one phase is known, that at the time of chromosome segregation during reduction division, but the possibility is not excluded that at other phases mutations can be induced in the genotype.

E. Ja. BORISENKO even goes further and points out that Lysenko demonstrated the possibility of regulating the development of organisms, and he is correct in maintaining that heredity does not consist only in the reduction division and gene combination.

S. S. PEROV, P. N. JAKOVLEV and D. A. DOLGUŠIN attack the conception of gene stability, because it contradicts the evolution theory; Dolgušin argues that geneticists have not sufficient data to attribute a hereditary change to mutation, as mutations are usually lathal and less resistant to adverse environment. Lysenko has disproved, with the data obtained in the research on "training plants", the conception that mutations are occasional and cannot be directed.

N. P. DUBININ, criticizing Lysenko, points out that the rôle of natural selection cannot be regarded as a fixation of directly induced mutations. Lysenko and Present, by attempting to modify the orthodox conception of segregation by introducing an idea of struggle and selection in hybrids between two contrary hereditary bases, follow the fallacious ideas of Weismann.

G. A. LEVICKIĬ attacks Lysenko on the same ground. He points out that according to Darwin, in the gamete formation not an adaptability, but a fortuitous combination takes place upon which natural selection operates; the conception of an adaptability will lead to Lamarckism. He draws attention to the fact that only the nucleus of the sperm effects fertilization; hence the cell, as a whole, cannot be regarded as the physical basis of heredity. While admitting a "functional" connexion between hereditary phenomena and chromosomes, Present must admit also the linear arrangement of genes.

G. D. KARPEČENKO similarly stresses the existence of a duality in the cell. The chromosomes are the basis of Mendelian segregation and sterility; incompatability of various species could be changed by doubling the chromosome number.

N. P. KRENKE considers that the disputing sides could reach agreement more easily if, in addition to their respective views, they referred to the opinion of leading geneticists, such as Morgan who seems to accept, to some extent, the evolutionary and ontogenetical variability of the gene. Morgan admits also such characters in the organisms which do not depend on the nucleus and are brought about mainly by the cytoplasm. He adds, furthermore, that examples of transference of plasm with the sperm are fairly frequent, but that these phenomena are not yet investigated fully.

During the discussion, considerable interest was focused upon the problem of using intra-varietal crossings.

A. D. ŠEHURDIN reports that the intra-varietal crossing as tested in Saratov yielded no appreciable results. The method of accelerated building-up of varieties (choice of plants from the fifth generation), as practised in Odessa, is virtually reproduction of selected plants, but not breeding.

B. V. SEMEVSKIĬ states that, on the contrary, the data examined by him show that the method of intra-varietal crossing is very promising, but at the present stage it must be further revised.

P. F. PLESECKIĬ, quoting his own results, considers that intra-varietal crossing, by increasing variability (adaptability) in the plants, improved and increased their productivity.

V. E. PISAREV, concerning the results obtained in intra-varietal crossings, notes that some varieties may increase in yield as a result of segregation in respect of physiological characters.

A. M. OLŠANSKIĬ remarks that in 1935 when Lysenko first suggested intra-varietal crossing, geneticists, referring to Johannsen's theory, predicted a failure; now, when positive results are reported, they attempt to interpret them in terms of heterosis. The same has happened with the method of 'training' plants.

E. Ja. BORISENKO holds that Lysenko justly criticizes the old genetical conception of inbreeding, because the gene, as well as the whole organism, is not rigid, and yet Lysenko is wrong when he raises his voice against some firmly established conceptions of cytogenetics.

A. A. SAPEGIN attributes the success of Lysenko's experiments on intra-varietal crossing to heterozygosity of the varieties dealt with and suggests checking the effect of this method by individual selections from F_3 and F_4. He considers that heterozygous plants must be more productive and suggests the

breeding of heterozygous wheats which, like rye, will retain their heterozygosity.

As regards the alteration of the plants by environment, the interpretation given by Lysenko is Lamarkism, as the adaptability is made up by selection. He points out that the action of the gene on development is not single, and that the hereditary base cannot be fully understood only in terms of chromosomes without reference to the development of the organism, just as the development cannot be fully understood without reference to the structure of chromosomes.

N. P. DUBININ acknowledges that Lysenko's research on phasic development is extremely interesting and the sooner Lysenko understands that development is based upon the factorial hereditary base, the better for him and science.

Some controversies have arisen regarding Vavilov's law of homologous series.

B. V. SEMEVSKIĬ states that the law of homologous series is a further development of the conception of Darwin who had noted a parallelism in variability in some related genera and species, and this law is based upon a comprehensive body of firmly established facts.

P. N. YAKOVLEV and N. G. ŠLYKOV on the contrary think that the law of homologous series and the theory of centres of origin of cultivated plants, as advocated by Vavilov, are

inconsistent with genetics as well as with Lysenko's theory. The law of homologous series is a perversion of those sound conceptions which had been formed by Darwin and must be revised as it has been based upon conceptions of Bates and Berg.

N. G. ŠLYKOV claims, in addition, that the theory of centres according to which all the genes are geographically centred, has not come up to expectations.

4. CONCLUSIVE REPORT READ BY G. K. MEISTER.

It is regretted that the discussion was not carried on in a friendly spirit and in a proper style. He emphasizes the significance of Darwin's work, but points out that several of Darwin's theories, e.g. pangenesis, proved to be erroneous; therefore, Darwinism today should be understood in the form in which it has been modified in modern science, otherwise investigators will surely be misled, as has happened with the Odessa workers who, referring to Darwin's opinion, attempt to defy inbreeding, as such.

Meister points out that Muller in his report took a prejudiced position towards Lysenko's and made up his report with a view to disproving Lysenko's hypothesis. Using a great number of experimental data, he centred his attention on proving the stability of the gene, and on the absence of directed variability (mutations) in gene. Though referring frequently to Darwin, he, by exaggerating the stability of gene left an impression that he repudiates Darwinism as a theory of evolution. Muller overlooked the fact that geneticists studied mostly morphological characters, physiological characters being left out. Bauer showed that in nature there are numerous minor mutations. Morgan in his latest book does not deny the possibility of a complication of evolutionary

processes of the genes themselves, although there is not yet experimental data to substantiate this suggestion. Speaking of genes, Muller did not mention that, apart from genes, evolution in the light of experimental genetics should also be explained by structural and numerical changes in chromosomes and hybridization.

Serebrovskiĭ made similar mistakes in exaggerating the formalistic conception of the gene; he failed to understand that breeding is not pure genetics, as it involves also some other yet unknown aspect of ontogenesis. Breeding at present is a selection as well as an art, and cannot progress without using the experience gained in practice.

Vavilov in his report forgot entirely the aim of the present discussion,and, instead of a critical review of his research, spent the time in enumerating the various advances made at his institutions. The theory of homologous series, later known as the theory of parallel mutations, led to the theory of the primary centres of origin of various cultivated plants, where the existing genes are supposed to be centred. This theory is based upon the atomistic conception of genes and tends to undervalue the formative processes going on in the secondary centres of cultivated plants. The law of homologous series served its purpose by establishing the regularity observed in variability, at least, in the main morphological characters, and

by giving to breeders a rich initial material for a number of plants, and yet there is a number of incorrect and fallacious views in Vavilov's theory, which, when handled by his collaborators, lead them to anti-Darwinism.

Meister, turning his attention to Lysenko's report, stresses the fact that Lysenko has already revolutionized plant breeding and plant physiology. At the present, no plant breeding can be carried out without referring to Lysenko's theory of phasic development; the method of vernalization has been adopted on an ever-increasing scale in practice; finally, the methods of accelerated breeding and reproduction of varieties are also successfully used. It is also to great advantage that Lysenko is enforcing a revision of conceptions which were so far regarded as stable. We have to learn in Lysenko's work how to convert a theory into practice; he has shown convincingly that intra-varietal crossing has shaken the conception of pure lines and, apparently, the use of this method will enable breeders to increase the yield and winter hardiness in plants. And yet it is hardly possible to agree with some of Lysenko's conceptions; such as the degeneration of pure line varieties; his criticism of inbreeding is also hardly sound, as inbreeding in certain plants is indispensable.

The results reported by Lysenko on the conversion of plants by training cannot as yet be theoretically explained clearly. This question, as well as the effect of intra-varietal crossing, must be studied further. The criticism of Lysenko's research, however, does not minimize his merits in the defence of Darwinism, as well as in the study of development of plants.

At the close of his report, Meister warns the scientists present that they should be very careful and precise in formulating theoretical conclusions, as theoretical views now quickly find their way into practice.

5. CONCLUSIONS.

It is obvious that the discussion following these reports could not bring agreement on those main points in which the views of modern genetics and those of Lysenko are in an apparent conflict. The real cause of this is a general misunderstanding. The geneticists during the discussion betrayed the fact that they have not an adequate knowledge of the results obtained and the hypothesis put forward by Lysenko. The fault lies with Lysenko, who so far has not published his results and conceptions fully. In his various reports he merely refers to them or reviews them so briefly and inexplicitly, that they may be interpreted in different ways; besides, he seems to undervalue the advantage of using scientific terminology.

This, to some extent, may account for the fact that geneticists did not follow the fundamental views of Lysenko, nor did they try to give the results reported a proper interpretation in genetical terms. Lysenko in the most important part of his theory was misunderstood; while maintaining that the whole cell is the physical basis of heredity, he means presumably that it is so functionally; changes within the cell influence the nuclear material, which, as a rule, is alone transmitted to the next generation. Therefore, geneticists should have

discussed the problem whether cytoplasm influences the nuclear material, or whether the nucleus can accumulate and retain the changes previously acquired by the surrounding cytoplasm.

The geneticists accused Lysenko of pure Lamarckism, yet Lysenko differs from Lamarck in assuming that the genotype at definite developmental stages can be influenced by definite environmental factors, and that the process consists in slow, continuous and gradually increasing changes (mutation), while Lamarck held that a genotype can be abruptly changed by an environment in general.

In the conversion of winter forms into spring and spring forms into winter, presumably some yet unknown complications and upheavals could be responsible, and it is only to be regretted that the study of "training" plants has not been accompanied by cytological research.

In the discussion dealing with the results of intravarietal crossings it should have been borne in mind that the majority of pure lines was established and maintained by morphological characters; physiological difference within a line is not excluded and may account greatly for the results obtained.

We should like to point out, in closing our colloquium, that Lysenko's results are very interesting, besides

being of great practical value, and it is only to be regretted that a fuller or complete account of his results and explanations is not yet available, which would enable us to examine them on statistical grounds.

Even more regret should be felt that the geneticists present in the congress and headed by eminent representatives of modern genetics failed to grasp the root of the differences, and left Lysenko and his co-workers in a position which will necessarily cause further difficulties and controversies in genetics.